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XXIII. *On the comparative Influence of Male and Female Parents on their Offspring. By* Thomas Andrew Knight, *Esq. F. R. S. In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read June 22, 1809.

My Dear Sir,

I have been engaged, during many years, in experiments on fruit-trees, of which the object has been to discover the best means of forming new varieties, that may be found better calculated for the climate of Britain than those at present cultivated. In this inquiry my efforts have been always most successful, when I propagated from the males of one variety and the females of another; and I was enabled, by the same means, to ascertain more accurately, than had previously been done, the comparative influence of the male and female parent on the character of the offspring. The analogy that subsists between plants and animals, in almost every thing which respects generation, induced me also to attend very minutely to similar experiments in which I engaged on some species of animals; and as the repetition of such experiments would necessarily require a very considerable space of time, and as the results seem to lead to conclusions that may be of public utility, I have thought the following account sufficiently interesting to induce me to address it to you.

Linnæus conceived, that the character of the male parent predominated in the exterior parts both of plants and animals;

and the same opinions have been generally entertained by more modern naturalists. But the Swedish philosopher appears to have been misled, by the striking predominance of the character of the male parent in male animals, and to have drawn his conclusions somewhat too generally: for I have observed that seedling plants, when propagated from male and female parents of distinct characters and permanent habits, generally, though with some few exceptions, inherit much more of character of the female, than of the male parent, and the same remark is applicable, in some respects, to the animal world, as I shall point out in the succeeding narrative.

My experiments were made on many different species of fruit-trees; but most extensively, and under the most advantageous circumstances, on the apple-tree; and as the results were all in unison with each other, it will be necessary to trouble you only with an account of some of the experiments which were made on that species of fruit-tree.

The apple, or crab of England, and of Siberia, however dissimilar in habit and character, appear to constitute a single species only; in which much variation has been effected by the influence of climate on successive generations: for the two varieties readily bred together, and the offspring, whether raised from the seeds of the Siberian, or British variety, were prolific to a most exuberant extent. But there was a very considerable degree of dissimilarity in the appearance of the offspring; and the leaves, and general habits of each, presented an obvious prevalence of the character of the female parent. The buds of those plants, which had sprung from the seeds of the cultivated apple, did not unfold quite so early in the spring; and their fruits generally exceeded, very con-

siderably, in size those which were produced by the trees which derived their existence from the seeds of the Siberian crab. There was also a prevalence of the character of the female parent in the form of the fruit; but the same degree of prevalence did not extend to the quality and flavour of the fruit; for the richest apple that I have ever seen, and which afforded expressed juice of much higher specific gravity than any other, sprang from a seed of yellow Siberian crab.

The prevalence of the character of the female parent in the preceding cases, may possibly be suspected to have arisen from some error, or neglect of accuracy in making the experiments; but I do not conceive that any such errors could have existed; for the trees of each variety were trained to walls, where they blossomed much before any others of the same species, and the stamina were always carefully extracted, whilst immature, from every blossom, which I intended to afford seeds. The remaining blossoms of the trees were also totally destroyed, and no other blossoms, except those from which the pollen was taken, were ever unfolded in the neighbourhood, in the season when the experiments were made; and I have also invariably declined to draw any conclusion from the appearance of a plant, in which I could not certainly distinguish some portion of the features and character of the supposed male parent.

It is perhaps also proper to state, that the predominance of the character of the female parent, could scarcely have arisen from any defective action of the pollen; for, except in cases where superfœtation took place, I have invariably found the effect of a very large, or a very small quantity of pollen, to be invariably the same, in its influence on the offspring; and

in the greater part of the experiments, from which I have drawn the preceding conclusions, more than ten times as much pollen was deposited on the stigmata, as could have been deposited in unmutilated blossoms by the ordinary means employed by nature.

In all attempts to discriminate the different influence of the male and female parent on the offspring of animals many difficulties present themselves, owing to the intermixtures which have been made of the different breeds of domesticated animals of every species, and the consequent absence of all hereditary permanency in the character of each variety. For under these circumstances, the offspring will be very frequently found to shew little resemblance either to its male or female parent, either in form, or stature, or colour. It will therefore be necessary, before I enter on the subject of viviparous animals, to observe that when I apply the terms large and small to the male or female parent, I extend the meaning of those terms to the parentage, from which the male and female descend, and not to the size of the individual only, which becomes the immediate parent of the offspring.

Mr. CLINE has observed, in a communication to the Board of Agriculture, that if the male and female parent differ considerably in size, the dimensions of the fœtus, at the birth, will be regulated much more by the size of the female than of the male parent; and, if the meaning of the terms large and small be extended to the varieties, as well as to the individuals, his remark is perfectly just. But experience compels me wholly to reject the inference that he has drawn respecting the advantages of propagating from large, in preference to small females.

Nature has given to the offspring of many animals (those of the sheep, the cow, and the mare, afford familiar examples) the power, at an early age, to accompany their parents in flight; and the legs of such animals are very nearly of the same length, at the birth, as when they have attained their perfect growth. When the female parent is large, and the fœtus consequently so, the offspring will be large at its birth, in proportion to the bulk it will ultimately attain, and its legs will thence be long comparatively with the depth of the chest and shoulders. When, on the contrary, the female is small, and the fœtus so, at the birth, the length of the legs of the young animal will be short comparatively with the depth of its chest and shoulders; and an animal in the latter form will be greatly preferable, either for the purposes of labour, or of food to mankind. I have seen this difference in the influence of the male and female parent, on the offspring, very strikingly exemplified, in the result of an attempt to obtain very large mules from the male ass and the mare. The largest females, that could be procured, were selected, and the forms of the offspring, at the birth, were perfectly consistent with the theory of Mr. CLINE; they were remarkably large: and I observed, that the length of their legs, when they were only a few days old, very nearly equalled that of the legs of their female parents. I examined the same animals when five years old, and in the depth of their chests and shoulders, they very little exceeded their male parent; and they were consequently of little or no value; whilst other mules, which were obtained from the same male parent (a Spanish ass), but from mares of small stature, were perfectly well proportioned. I have never seen the little mule, which is propagated from the

female ass and the horse, nor even a delineation, or description of its form; but I do not entertain any doubt that its chest and shoulders are excessively deep and strong, comparatively with the length of its legs, and that, on account of this peculiarity in its form, it has been so frequently shewn on the Continent, under the name of a jumart, as the pretended offspring of the mare and the bull.

In opposing the theory advanced by Mr. CLINE, it is not by any means my intention to enter the lists with him, as a physiologist; but, as a farmer and breeder of animals of different species, I have probably had many advantages, which he has not possessed; and my conclusions have been drawn from very extensive, and, I believe, accurate observation.

There is another respect in which the powers of the female appear to be prevalent in their influence on the offspring, and that is relative to its sex. In several species of domesticated, or cultivated animal (I believe in all), particular females are found to produce a very large majority, and sometimes all their offspring of the same sex; and I have proved repeatedly, that, by dividing a herd of thirty cows into three equal parts, I could calculate, with confidence, upon a large majority of females from one part, of males from another, and upon nearly an equal number of males and females from the remainder. I frequently endeavoured to change these habits by changing the male; but always without success; and I have in some instances observed the offspring of one sex, though obtained from different males, to exceed those of the other, in the proportion of five or six, and even seven to one. When, on the contrary, I have attended to the numerous offspring of a single bull, or ram, or horse, I have never seen

any considerable difference in the number of offspring of either sex. I am therefore disposed to believe that the sex of the offspring is given by the female parent; and the probability of this seems obvious in fishes, and several other species of animals which breed in water; and though the evidence afforded by the facts adduced is not by any means of sufficient weight to decide the question, it probably much exceeds all that can be placed in the opposite scale.

In oviparous animals, I have had reason to think the influence of the female parent quite as great, as amongst the viviparous tribes, though my observations have been more limited, and less conclusive. In viviparous animals, the size of the fœtus is affected by the influence of the male parent, and, in some instances, not inconsiderably; but the size and form of the eggs of birds do not appear to be in any degree changed or modified, by the influence of the male; and therefore the size of the offspring, at the birth, must be regulated wholly by the female parent; and this circumstance permanently affects the form and character of the offspring. The eggs of birds, and those of fishes and insects (if such can properly be called eggs), appear to resemble the seeds of plants, in having their forms and bulk wholly regulated by the female parent; but nevertheless their formation appears to depend on very different laws. For the eggs, both of birds and of fishes and insects, attain their perfect size in total independence of the male, and the cicatricula, the vitellus, and the chalazæ have appeared (I believe) to the most accurate observers, to be as well organised in the unimpregnated, as in the impregnated egg: in the seed, on the contrary, every thing relative to its internal organisation appears dependent on the male parent. SPALLANZANI has,

however, stated, that many plants produced well organised seeds, and even seeds which vegetated perfectly, under circumstances in which it is not easy to conceive how the pollen of the male plant or flower could have been present. But the Italian naturalist appears to have blundered most egregiously in his experiment; or (which I conceive to be more probable) he became the dupe of the refined malice of his countrymen; for, I repeated his experiments under very favourable circumstances, and with the closest attention, but I failed to obtain a single seed. The gourd alone produced apparently perfect fruit, and the seed-coats acquired their natural size and form; and in this respect the growth of its seeds appeared to be, like that of eggs, wholly independent of the influence of the male. But the *seed-coats* of the gourd were perfectly *empty*, and I could not discover, at any period of their growth, the slightest vestige either of cotyledons, or plumule, nor of any thing that appeared to correspond with internal organisation of a seed of the same plant, under different circumstances. SPALLANZANI has not, I believe, mentioned the species of gourd upon which he made his experiments: the common, or orange gourd of our gardens, was the subject of mine.

In comparing the mode of the formation and growth of eggs with the observations I had previously made on the growth of seeds, I have been favoured with the very able assistance of Mr. CARLISLE, for which I have on this, as on many other occasions, to acknowledge much obligation.

I am, my Dear Sir,
with great respect, sincerely yours,
THOMAS AND. KNIGHT.

Downton, May 20, 1809.